

**Interview with Gary Becker**  
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Claudia Goldin: This is Claudia Goldin, and it is August 5th, and I'm in my Littauer office with Gary Becker of the University of Chicago and with my friend, Larry Katz. And we're going to talk...

Gary: Of Harvard University.

Claudia: What? Of Harvard University.

Larry: And the year is 2003. Make sure we've got it all documented correctly.

Claudia: And the year is 2003. And we're going to talk with Gary about... mainly about the Bureau. But he can meander to whatever he wants. So you were talking about something having to do with the history of the Bureau. You said something about knowing the history of the Bureau through up until... I wanted to continue that.

Gary: Up until it left...

Claudia: Yeah.

Gary: New York, and I think John [Meyer] moved it to New Haven for a while...

Claudia: Right.

Gary: And then eventually to Cambridge, but I first became familiar with the Bureau through Milton Friedman when I was a graduate student. So I knew a lot about the Bureau, for a graduate student anyway, while I was at Chicago, and its great empirical tradition which the Bureau was. I mean, [Arthur] Burns and [Wesley] Mitchell are basically anti-theoretical even though Burns taught a course at Columbia on theory, but he taught it in the form of, you know, "Look at these assumptions. Look at these assumptions," in a negative fashion. And it turned people off of theory, even though Burns himself was a competent theorist, a Marshallian type.

So the Bureau developed a strong tradition, by the time I came there, of very empirical, very careful work, which was not the standards of the profession at that time, which was largely non-empirical, or if it was empirical, it was sloppy empirical. NBER always said if you had a research assistant and they had to compile data, they would have another research assistant who would be looking at the data and the other person would be reading it. It was a very careful procedure to make sure of accuracy. And as I say, I think one thing at the Bureau in those days, it was a great

emphasis on quantitative work that was accurate, that you could trust, and I always would say any number that Geoffrey Moore, for example, had told me, I would have a 100 percent confidence in that number. He wouldn't give a number unless, you know, he had checked it very carefully. But the downside of that, from my perspective, was there was kind of an anti-theoretical orientation at the Bureau, and that eventually began to change. I'll stop here. Maybe you have something you want to direct me to. I could go on.

Larry: I guess the question on that point would be, obviously the person who linked the theoretical and the empirical projects was Milton Friedman, your advisor, was the fact that he was Chicago and not that involved there day-to-day meant he didn't affect what actually happened at NBER because clearly, his projects, coming from *Independent Professional Practice* to *Monetary History*, clearly combined the two.

Gary: Yeah, his *Income from Independent Professional Practice*, as you know — I don't know if you know the history of that — that started out as a study by Simon Kuznets, because with some data on doctors' earnings from '29 to '33, I think approximately. Milton Friedman was brought in, and it became his doctoral dissertation, and you can see even in the early... in the introduction to the independent stuff in the book that, say, what Friedman did and what Kuznets did, and Friedman brought into the study a lot of analytical techniques, in particular, I would say, two things in particular were the human capital analysis, so we look at discounted earnings, and he introduced a permanent/transitory distinction which started there, and then he applied it to the consumption function.

So I always thought the study on *Income from Independent Professional Practice* was perhaps the greatest empirical study of its time in the sense that it was careful, empirical, and it had an analytical analysis. And it was a great book. I mean, it was terribly reviewed in the *American Economic Review*. I once checked that when I wrote an essay about Friedman. "Why waste your time on this little problem?" was the basic review. It never got the reception that it deserved, but to me reading it as a graduate student, I always thought it was a great book, and I think it stands as a really great book to anybody who comes across it now.

As you said, Larry, he combined theory and empirical in not only that book but, to some extent, in his monetary history with Anna Schwartz. Anna was the more empirical person. She was a very careful worker, knew monetary history very well. Milton really, until very late, didn't really know monetary history that well. He learned it in the course of doing it. Anna knew more.

Claudia: I think Anna learned it in the course of doing it.

Gary: Yeah.

Claudia: I think that there was this magic moment when they thought they finished the book, brought it to Princeton, had it read by someone, and the person said, "This could be a monetary history." And then Anna went and got out of the library, you know, hundreds of books, read them, summarized them.

Gary: Yeah, it could be. I don't know. You would know that better than I do. But she

eventually knew more of monetary history than Milton. But Milton had a fair amount, and I think, partly indirectly through Anna really and maybe partly on his own reason. But when I came to the Bureau, the way I came to the Bureau — that might be relevant — I was thinking I didn't have a lot of offers; I had one from Columbia and I wanted to be on the East Coast. I really only had Columbia. Columbia made me an offer.

Claudia: This is in '56 or '57?

Gary: '56, I think I went there in '57. I'd been assistant professor at Chicago for three years, and Chicago actually offered me more money to stay. But I wanted to leave Chicago to get on my own and so I wanted to be on the east coast for various family reasons, and it wasn't a huge market. I mean, Johns Hopkins, I interviewed with, they decided not to make me an offer but Columbia did, and then I sort of through my connection with Milton, the National Bureau came in and I said I would like to be connected with the National Bureau. It was important to me as part of Columbia. And they asked me what I would like to work on, and I said I would like to work on calculating rates of return on education.

So what I suggested was a very empirical project. And Sol Fabricant, who was then director of research, went out and raised \$37,500 from the Carnegie Corporation with John Gardner. I don't know if you know that name. He was the head of the Carnegie Corporation. He was always very proud that you supported this early study. It was a very small grant but it was just for me and our research assistant, a little... They gave me \$3,000 a year as salary. And it started out entirely as an empirical study. At Chicago, Ted Schultz was working on human capital, so human capital was known. There was some work by some others going up. I thought the empirical work on that had not really been done carefully enough in terms of estimating the returns. So I started getting data and thinking about it, and the more I began to think about the problem, the more it occurred to me that it offered a fairly unified way of understanding a lot of labor market issues. So I began to think of on the job, not only its role of education and earning, but also on-the-job training, general and specific training, who paid for the training is an issue I got into, some stuff from the military, the consequences for inequality, a lot of theoretical issues that I could see opened up, that this approach opened up, so I altered the project.

I still had, for me at least, a big empirical section on estimating the rate of return. The first half of the book was basically theory, and then the second half of the book was testing the theory. But mainly it was empirical that had some relation to the theory, because a lot on the job stuff, I didn't try to test. It took a lot of time before we gained... I don't, still, know that much about it. But I did do a lot of work on the empirical side, and the first paper I reported out on that book, Arthur Burns asked me to present a paper at the meeting of the American Economic Association on some of the empirical results, which I did do, and it was eventually published in the proceedings, called *Underinvestment in [College] Education?* I made calculations of the rate of return on education and compared it with some data that [George] Stigler had put together for the National Bureau and the rates of return on physical capital. And I concluded there was no obvious evidence that was underinvestment in education. I looked at the externalities issue; some tried to make some calculations on what it could be and if there were these externalities, where they would be. So I

did a little work on that at the time. Mainly, again, empirical, but a little bit of a theory, and then we had this habit, and I got encouraged. I wasn't discouraged from working on the theory, even though the Bureau — let me give you a little more about the Bureau — when I would go to the Bureau certainly every Friday and often a second day of the week. I would only go to Columbia like three days a week. I lived out in the suburbs, and I would go at least one day, often two days to the Bureau. So I got to know everybody at the Bureau.

Claudia: The Bureau was at Columbus Circle?

Gary: The Bureau was at 39th and Madison. I don't know where that's... No, that wasn't in Columbus Circle.

Claudia: Yeah.

Gary: The Columbus Circle was the original. No, eventually they moved into this fancy office building. They had, I think it was the 11th floor, and they had one whole floor of an office building. It was just an office building really, and they rented. They had a very good long-term rental contract, so from my perspective was good. I would take the train. You could take the train from the suburbs — I was living in Scarsdale — and you'd be at Grand Central, very close to the Bureau. So I would go in a lot. I got to know the people there. I interacted with some of them very, very closely.

Larry: So which people?

Gary: Who I interacted with? Well, of course it depends. The years are a little bit merged in my head.

Larry: Yeah.

Gary: But the people I were close with [included] Jacob Mincer. He spent one year while I came first to the Bureau. He spent the year at Chicago as supposed to Schultz bargained with him too tightly and he decided not to take... He was offered a Chicago assistant professorship, and he decided to come back to New York, where his wife had a good position at Montefiore Hospital. He and I would interact a lot, even when he was at City College, and then we brought him into Columbia. We interacted even more. We ran a workshop together. It was a very good workshop. And we were at the Bureau together, so... We never published anything together, although we did do a paper together which we should have published. It was kind of a perfect, kind of, a rational expectations model of the consumption function, but limited by the assumption of perfect certainty, that people could look ahead.

Larry: Something like a perfect foresight.

Gary: Perfect foresight model of consumption, and I was a little bit bothered. We didn't quite get the timing right, so we put it aside, but we should have published the paper. It was a really, as I look at it in retrospect, a very good paper. That was the only thing we ever did together, but we indirectly...

Claudia: Jacob told me a little bit of that.

Gary: Pardon me.

Claudia: Jacob told me a little bit about that.

Gary: Jacob? Oh, he did? Okay.

Claudia: He said that he — that you divided it up. Now you must have both been in New York at the time but somehow...

Gary: We were both in New York.

Claudia: He mailed you a copy, and maybe the mails weren't working very well.

Gary: I can't remember.

Claudia: Yeah.

Gary: I can't remember. Not too long ago, I found a copy. No, it's something we certainly should have published. It would have been a good take...

Claudia: But then Bob Hall published it.

Gary: Bob, yeah, he had uncertainty in it, but basically it was the same sort of thing with... Uncertainty's relevant, of course, for these problems. A lot of the rational expectations stuff you get the essence out of it in perfectly certain situations. Uncertainty adds interesting questions, because the world is not perfectly certain and things don't fully explain... Anyway, that's what we had, so we never wrote anything, but we interacted a lot.

Victor Fuchs, when I first met Victor Fuchs, he was teaching at General Studies. He seemed like... He was a Columbia Ph.D., one of these big nice guys, who I never took very seriously, to tell you the truth. I liked him. I mean, he was a nice person and then he went to the Ford Foundation for two years, and usually you get ruined by going to a foundation, but he came back much better than ever. And he went to the Ford Foundation not as a research fellow but as a program officer. That's what he was doing. But it led him to think about a lot of problems, health and so on.

Claudia: Right.

Gary: And he came back as a ball of fire after that. He had a lot of ideas, brilliant ideas. So he and I began to interact a great deal. I got some money. I don't remember how I brought in some education fellows for a year. Sherwin [Rosen] came in for a year, got to know him. Albert Fishlow, you wouldn't think of associating him with it, but he came in. He was working on 19th century education at the time and I brought him in and I interacted with him a lot. And I'm still on good terms with Albert, although his interests have shifted. [Zvi Griliches] came in as a different fellow for years. I interacted with him closely.

Claudia: This is in the early '60s?

Gary: This was in the '60s. I can't remember the time. I came to the Bureau from '57 and I left around '69 in terms of physically, and so it took place over that period. Sherwin probably came in in the early '60s to the Bureau, so he came in the early '60s. We made Zvi an offer at Columbia, but he decided to go back to Chicago. He didn't accept it. Rosen went back to Rochester. Rosen was going nowhere. They were about to dump him. This might be relevant for your study, because his work in the National Bureau really got him excited. He did this book with [Ishaq] Nadiri on dynamic models of the labor market. Maybe it wasn't great, but it was, for that time, it was innovative. Got in, had a lot of other ideas, so he went back to Rochester brimming with ideas and eventually he became one of their stars. So they made him the chair and so on. But he was very close to being thrown out of Rochester. His work was going slowly, so Sherwin was slow and the Bureau gave me a great period for him to — he always said it was a great period for him. And he came to our workshop; I think I had a big influence on him at that time. And gradually the Bureau started to change a little.

I proposed to Arthur Burns and sometime in the early '60s, we start a program and wanted in the mid-'60s. He turned me down, Arthur Burns, you asked me somewhere about Arthur Burns. He and I, he was like a mentor to me. He liked me a lot, gave me good advice, he socially invited me to his house and he came to our house, and when you went in to see Arthur Burns, it was like going into a black hole. You were there for an hour and a half or so, and he loves to talk and so on. You know, sometimes I'd be trying to catch a train to go back and he keeps on talking and talking. But he was a very important influence on me. I liked Arthur a lot. He was a very smart man, and he was very sympathetic to my work, but he didn't want [all of it at] the Bureau. He had the old conception of the Bureau: Stay within the confines. He was not so much about expanding economics the Bureau would deal with. So while he was quite receptive to the human capital work, you could see that, but the law and economic stuff he turned down. We did eventually get, I think, the first law and economic program anywhere in the world, which I did start, and then we brought in two major people on it, Bill Landes and Dick Posner, and they were both connected with the National Bureau law and economics project, and we had other people. Isaac Ehrlich was a graduate student working on it.

Claudia: So how did you manage to do this, given that Burns was against it?

Gary: Well, he was first against it, and we came back again with now Fuchs supporting it and then they changed their mind. Exactly why, I can't... I don't know what went on in Burns' mind. Maybe when he could see this was a field rising, he became more convinced of it. We had Fuchs, who wanted to do health economics. We started the first, maybe, health economics project. Not me, it was Victor who started that, and I think a very dynamic project going on there on health economics. And eventually we put it together, a center for... I forgot what it was called, economic and social something, and Victor Fuchs became the first director of it. Victor, me, Jacob, Bill Landes, Dick Posner, Finis Welch... I had a great collection of people in it who all had some affiliation with the Bureau, some of them through the graduate program at City University, where Victor finally went, where Finis Welch went, where Landes went, Mike Grossman came, Jim Smith and a couple of other people.



It really was a top group for about four or five years when it completely disintegrated but at one time, it had a really outstanding group of people, all of whom pretty much were connected also with the NBER, and the Graduate Center was located on 42nd Street in some old department store or something. I don't remember what they took over. They took over some building, you might know better, but it was some building they took over. It was very close to the NBER, which was 39th and Madison, and they were 42nd around 5th and 6th. I don't remember where. It would be easy for them to come over. It was much harder to come from Columbia down, but occasionally I would do both on the same day and take a taxi down from Columbia to the Bureau. Well, I can go on, but maybe you had something specific you want me...

Claudia: No, I think going on is very good. We talked about a lot of the people that you spoke with at the Bureau, Finis, for example, so Finis was around there quite a bit.

Gary: He was around, yeah, toward the end of my stay there, but he was around, yeah, definitely.

Claudia: Right, and of course Victor and others, and then there was this group of people whom you brought in through this raising of some funds, like Sherwin.

Gary: Yeah, we had some, I forgot how we got them, but we had some postdoc funds, yeah. Exactly.

Claudia: Right, and that was very good. And occasionally you would talk to Burns, and Burns was very supportive of the human capital stuff... Much of the Bureau at New York was involved in the business research stuff, but they were sort of separate, and you didn't really talk to them. You...

Gary: When I came, pretty much all the Bureau was in business cycle stuff.

Claudia: Right.

Gary: Pretty much all.

Claudia: Right.

Gary: I knew them all and I would interact with some of them.

Claudia: You remember Ruth Mack?

Gary: Of course I knew Ruth Mack. She had done an early study on inventories, I think, I think so.

Claudia: Boots and shoes and leather.

Gary: Yeah, I knew Ruth Mack very well. So she would come in. She had a little independent income, so she was kind of well-to-do. She would come in. I know her

very well. I would interact with her. She was very empirical, very empirical. I mean, she didn't... I mean, there are a bunch of people, I would say, didn't really know economics in any solid way. Thor Hultgren, you may never have even heard of Thor Hultgren.

Larry: Oh, yeah.

Claudia: Right, I've heard of Hultgren.

Gary: Very empirical. Thor Hultgren was also... The Bureau was a great place for misfits. Thor Hultgren, as a person, was a misfit. He couldn't communicate. He couldn't interact with people, but he was great for the Bureau. The great thing about the Bureau, I think it has to be said, was the Bureau was willing to hire minorities of all types; Jews, you know, they gave a place for Jews. Burns was Jewish and Wesley Mitchell was a very fair-minded person, and he didn't care what your background was. A lot of Jews found employment at the National Bureau. Sol Fabricant was director of research when I was there. He was a very good guy. I liked Sol a lot.

Claudia: Simon.

Gary: Pardon me? Simon, of course. He was already in Pennsylvania when I was there.

Claudia: Right.

Gary: But he would come in, so I got to know him a little bit. I can't say very well, and I had a little run-in with him that he wasn't happy about it which we can go into... I'm not sure how much I'll mention, but of course I knew his work and had a lot of respect for him. Milton told me something about him, from the Bureau, and he would come in occasionally, but I didn't interact. Ray Goldsmith, you know that name?

Claudia: Yeah, I know Ray well. Well, I knew Ray.

Gary: He came in a lot, so I got to know him pretty well.

Claudia: Right.

Gary: He was doing these wealth estimates.

Larry: I guess the other person who I don't know where he was, but he published a book at least during your period that seems to possibly relate was Clarence Long.

Gary: Yeah, Clarence Long, I knew him slightly, because he may have been working... He eventually became a member of the House of Representatives... [left] academia, but I knew his book and I knew him, I don't remember. I think he started his book quite a bit before I actually got there. May have come out in publication...

Larry: It came out in '60 or so.



Gary: Yeah, but it had a long gestation period, and he was working on that. I knew about his work. I read much of his book, and I thought it was very interesting. I think it had an influence on Jacob, who as you know, Jacob's famous paper.

Claudia: An enormous influence. You know Jacob, when he says he went to Chicago, Al Rees... He took Al and Greg's labor course, because when Jacob arrived to Chicago they declared that he was a labor economist, and he said, "But I don't know any labor," and considered it an insult and...

Gary: It was insulting in those days, but I'll tell you a story about that, too.

Claudia: Yeah, so he took their courses, and Al Rees spent much of a semester talking about Clarence Long's work and...

Gary: Oh, did he? I didn't know that. Okay.

Claudia: And he and Greg, well, maybe this in a workshop because the way Jacob says it, he and Greg, Al and Greg, would debate, "How this would be? How could we resolve this?" And Jacob said that this was one of the magical moments in his life, when he said, "Oh, I can resolve it."

Gary: Could be. I don't know. I wasn't there at the time.

Claudia: Right.

Gary: But certainly, there were people at Chicago familiar, because Margaret Reid's doctoral dissertation dealt with household production in an important way, which was really an unsung book, I think, a good book in a lot of ways. But let me just tell you this briefly, tell you this story about labor economics in Chicago. When I came to Chicago, I had come from Princeton, which had an industrial relations tradition. Richard Lester, mainly, who I liked a lot, but was very empirical, which was good because [much] of the profession wasn't empirical. But didn't really know a heck of lot of economics, I would say, but did these empirical studies of where the workers got their marginal product, things of that type that I think were valuable in the profession, although they didn't get a lot of credit.

Anyway, labor economics at Princeton, all the worst students went into labor economics, so I didn't take any labor economics courses. Even when I came to Chicago, I sat in on Greg's course. I didn't take it, but I did sit in on it religiously, and I began to see that it was a very important course for me, because it was analytical as well as empirical. It was a combination. But when I was in my second or third year, Al Rees came to me, and I had started working on discrimination, and he said, "Well, Gary, we need an industrial labor center. We'd like to offer you a fellowship." I'd had a fellowship. He said, "Well, we'd like to offer you another one, because you're now working in labor economics." I said to him... I got really upset. I said, "Look, I'm not working in labor economics." I did tell him that. I said, "I'm working in applied price theory." Al was very nice. I mean, what I said was stupid. He said, "Whatever you want to call it, we're willing to work with you." [laughs]

It turned out when I went to Columbia. I was replacing Leo Wolman teaching labor

economics, which I did do for one or two years only, and then Jacob came and did it and I didn't teach it any longer. But at the Bureau what I began to see... I knew all these people. Thor Hultgren, C. Harry Kahn, Ruth Mack, Anna Schwartz...

Claudia: Victor Zarnowitz?

Gary: Victor Zarnowitz, knew him very well. Anybody you could mention that was at the Bureau I knew them, because I'd go in a lot. I'd go to all the seminars. Sure I knew them all, and I was on good terms with all of them. And my first seminar I gave at the Bureau on my own, I made it very, very empirical and I thought it was a poor seminar because I was... I felt I wasn't doing my comparative advantage and they weren't that into... They started telling me, "Well, what are some of the bigger issues that you were talking about? I mean, which was what I was interested in, so after that I learned my lesson and I spoke what I wanted to.

But the Bureau was very empirical, had some very good people, very careful as I said, but for my taste not enough analysis, and so I began to push as I got increasing influence, to have more analysis, so on the first philanthropy study they had with Frank Dickinson, which was very empirical, I wrote a little paper on how to look upon this from a theoretical point of view with altruism and things of that type. And when they had any other areas, I'd say, "Well, look at the theory of this." In my own work I began to report on some more theoretical aspects, and I found that these people were interested in that even though they were highly empirical, so the Bureau maintained its empirical bent while it was in New York but gradually also began to allow some more theory.

It was still measurement and careful measurement, but more theory was involved. They approved the law and economics project. They approved the center that Victor headed up, not only approved it but they thought it was good for the Bureau, and the business cycle stuff began to become of relatively less importance. At the Bureau, it still had these people working on it. It was still important, but its expansion was in other areas. It's not so much that they contracted that work, but they began to expand in these other areas, so from a relative point of view, it became definitely less important.

Larry: So I just have a question about a couple of projects in the Bureau that seemed... There's this 1960 volume in which you're first — at least what I think of as the first — paper on the economic theory of fertility comes in.

Gary: Oh yeah.

Larry: What's the story of that volume and your involvement in it, and was that done separately, or was it part of a Bureau project?

Gary: My paper was not really officially part of any Bureau project. I had no fertility project at the Bureau. It's something... I was actually... When I came and suggested I work on human capital for the Bureau, for me this was just a project to get the Bureau their support. My first interest at that time was fertility and population. Of course, my interest... I have a stated interest in that problem, but I gradually became really interested in human capital, which was sort of a way to earn money and be

connected with the Bureau. So that, the fertility stuff I did on my own. Now it was one of these Bureau conference volumes, but a Bureau conference volume had a different type of life than a regular Bureau volume. A regular Bureau volume was done at the Bureau.

You had to go through this difficult Bureau process, which held up the *Income from Independent Professional Practice* for several years because of this one board member who didn't like the conclusions about the medical profession, held up George Stigler's work on engineers because of some work and so on. But you had to go through a good process, a difficult process. The conference volumes were of a different nature.

I don't remember how I got into doing this, but I did this paper on fertility. The conference was held at Princeton actually, not physically at the NBER. I went down to Princeton and the paper was not well-received. I mean it was attacked a lot by [James] Duesenberry, who was my discussant, by most of the audience, and by... I had two defenders, Friedman and Kuznets. I'm pretty sure Kuznets was at that conference. I know they defended that this wasn't stupid, you know, and so it was well worth doing, but everybody else, economists as well as sociologists, were very much opposed to it. So, but I wouldn't identify that with a Bureau project. I may have done it on Bureau time. I didn't keep track carefully but it was never sponsored by the Bureau. None of my population work was ever officially sponsored by the Bureau, though it came out in this NBER conference.

Larry: And then there's this, in '62, this amazing JPE [*Journal of Political Economy*] issue...

Gary: Okay.

Larry: Where your work on human capital, Jacob's, George Stigler's sort of human capital approach to thinking about information in the labor market, [Larry] Sjaastad's paper on migration...

Gary: I know a little bit about that.

Larry: Which is listed as a Bureau... something linked to the Bureau. What was the story on that?

Gary: It may be. But I would say, I would give credit for that more to... Wasn't Ted Schultz the editor of that volume?

Larry: Yeah, he was the...

Gary: I would give him the credit for putting it together. It may have been some Bureau connection, but again it was not a Bureau project.

Claudia: Was it in the University's National Bureau series?

Larry: I think it was in the university's for research, because I know it had the NBER logo on the front.

Gary: Yeah, in the university, yeah. That's what it was. Those were done differently. Yeah, my work was real Bureau work. Jacob's work was real Bureau work. Jacob got interested in that through our workshop really. I presented some of the theory, and he saw how to get some measurements out of it so he had this important paper there. Sjaastad's work on migration had nothing to do with the Bureau. George Stigler's work on information, he never, as far as I know published any of that really for the Bureau. That was independent work that he did at Chicago. Selma Mushkin had some work, right, on health.

Larry: Right, on health.

Gary: Health. Which is not a Bureau work, but I would give Ted a lot of credit for putting that volume together, and it was not officially a Bureau volume, although it is in in this university's NBER, so the Bureau had to be willing to go along with it in some sense.

Larry: The university's research conferences are about other people.

Gary: But a good fraction of that volume was Bureau work. You look at pages probably were close to half that volume or something like that, so it was in that sense an important part but it was Ted Schultz. I was in constant communication with Ted when I was doing that human capital stuff. We had a lot of interaction back and forth, and I got a lot out of Ted. I gave a couple reports in Chicago. Ted was unhappy with my first paper on underinvestment in human... claiming there was, and that he had obvious evidence of underinvestment in human capital and he said, "Oh, you're just trying to be too free market."

I never saw Ted get annoyed at me. He really... Ted always... I was one of his favorite boys. He treated me very well, and I brought it back. "Look, Ted, this is what the evidence looks like. I don't feel I'm biasing in any way. If you can show me anyway where it's biased, and I'll be glad to change it." And he eventually bought a lot of it, but his initial reaction was very negative. But Ted Schultz was very important for me, for that volume. Clearly he put it together. And while he was not connected with the Bureau, as I said, and to a lesser extent Jacob, but I was in very close contact with Ted Schultz over that whole period of time.

Claudia: George was at the Bureau at the same time, right?

Gary: Well, yes and no. George was one of the reasons I came to Columbia. As I said, I didn't have any other offers, so I didn't have a big choice.

Claudia and Larry: You had Chicago.

Gary: Chicago, yeah, but I wanted to leave Chicago. Although if I had no offers, I would have stayed in Chicago, but one of the reasons was George Stigler's being there. I didn't know him well, but through Milton I had known George. I sent George a copy of my discrimination work, and he gave me one useful piece of advice which I did follow, an important piece of advice. But I admired him a lot, and he was a big attraction. He happened to be on leave when I came to Columbia at the center, and

he never came back to Columbia. He went from the center to Chicago. I remember I wrote him a letter, maybe on the presumptuous side. I spoke to him about it later. I said, "Look George, if you come back to Columbia, I will stay at Columbia." He eventually told me — this is maybe off the record, because it's tooting my own horn a little bit. I don't want to — but if he had known how important I would become, he might have come back.

Claudia and Larry: [laugh]

Gary: I don't think it had any influence on him. Columbia made a terrible mistake in bargaining with him, as Columbia made many mistakes, and I think his inclination was to come back, but he got a very good offer from Chicago, and he would have been willing to accept less money from Columbia, but they kind of insulted him in a way, and he didn't come back. So I never interacted with him, so from that point on, he had a, maybe for a while, a Bureau connection but a distant Bureau connection. He was affiliated, I'm sure, from several years subsequently, maybe many years, I don't remember, but he didn't come in physically very much. I corresponded with him. I knew him, and I would send him a lot of my stuff, and he would send me comments on them, but not because I physically interacted with him much in New York. Only very occasionally would he be in or would Milton be in. Milton would come in very occasionally to talk about the monetary history with Anna, but not very much.

Claudia: Summers.

Gary: A little bit in the summers. He'd go to New Hampshire, and he'd come in, so he wouldn't be around a lot. But when he was around I usually met him. Although Greg was the chairman of my committee. Greg and Milton were the two people I was closest to. So I was very close to Milton, so I would make an effort to see him, and he'd see me when he came in. He visited Columbia in '64. We tried to get him to stay. He thought about it seriously but decided not to stay and went back to Chicago at that time, so he spent a whole year in New York, and he was at Columbia more during that year.

Claudia: What about students? Columbia students working as research assistants. So the Bureau always had this tradition of having research assistants, many of whom were not graduate students. They were just smart undergraduates who came on and went to graduate school later, but this was... It seems to me that this was — and tell me if this is the case — a new transition in which you had your graduate students who were working on their dissertations, working at the Bureau, rather than people like Bob Lipsey, for example, who was a Columbia graduate student but he needed full-time money so he was working at the Bureau.

Gary: Yeah, became full-time at the Bureau. Yeah. I don't know if I started the tradition, but I certainly was one of the prime pushers. I thought it was great for graduate students to be connected with the Bureau, to get involved in a project. Sometimes they could get their thesis out of it, or it would be related to their thesis or just be an interesting project, so I began to bring pretty much all my top graduate students in to work at the Bureau. I was able to get them positions. If you think of the students I had, pretty much all of them. Isaac Ehrlich, Gilbert Ghez, Mike Grossman, Bob Michael, Bill Landes, Barry Chiswick, I mean, all these people I'm sure and some others worked at the National Bureau, and some others.

Larry: A number of them had major Bureau books and dissertations.

Gary: And some of them came out with major Bureau books, like Bob Michael and Mike Grossman each published his monographs, which were really extensions of their dissertations. Mike's stuff on Health Capital was his dissertation at Columbia, he did partly at the Bureau, and Bob's study on productivity in the household sector was his dissertation at Columbia. Isaac, in his law and economics book that Landes and I edited for the Bureau, Isaac had one of his papers in there, so he definitely worked at the Bureau. Ghez, well, Ghez and I, the book we did, I don't know... Was it a Bureau book?

Larry: I think it's a Bureau book.

Gary: Yeah, I think it's a Bureau book. He worked at the Bureau,. Ghez was actually ... he's one of the unfortunate cases. He was one of the top students of his cohort, and he just never... He had too many mental problems and he just never made it, but he was a really good student and he was there. Arleen Leibowitz worked at the Bureau. I'm sure a bunch of others. But we did that. Jacob did it too. Arthur Burns never really did it. He didn't have any graduate students when I was there, to speak of, so yeah, we began to do that and it was great for the graduate students,. It was a great experience for them and it was good to get good research assistants and in the areas where we were working, instead of getting these outside undergrads. My first assistant at the education human capital study was just a smart young girl who was a graduate of one of the seven women's colleges...didn't know any economics, but she was a good assistant to me.

And that was the tradition then. You got these people. Some of these research assistants were 65 years old. If you looked at the people working around the Bureau, there would be these old people, a lot of women. The Bureau was an employer of women when women couldn't get good jobs, so again, this hiring of minorities. One of the important things to be said about the Bureau was they hired women, and not just, you've mentioned Ruth Mack, Anna Schwartz, and there were other women who were regular researchers but there were a lot of older women working there, many of whom I got to know...

Claudia: Edith Kuznets was a research assistant there, and that's how she met Simon.

Gary: Yeah, it could be. I didn't know her.

Claudia: Well, that was long before.

Gary: Gerhardt Brie, did you know the name Gerhardt Brie? He wrote some empirical studies for the Bureau. You may not even know. He worked very much with Brie, and there was a woman, forgot her name now, who worked for years at the Bureau who was a coauthor, was more than just a research assistant, became a coauthor with him on that, so she was there a long time. Anna had some old-timers working for her, Victor Zarnowitz. There were a lot of old-timers working there who had been around for years. I mean, many of them, as I said, could not fit in with a



regular business-type organization, so the Bureau hired misfits. This may sound strange, but it was great for the Bureau. They were cheap. They were often good, and you know, it gave them jobs and it was good for the Bureau. So the Bureau, in that respect, was a very important organization. Wesley Mitchell set the tone on that. Wesley Mitchell was a very fair-minded...I didn't know Wesley Mitchell, but he was a very fair-minded, from everything I heard, honest individual and he was willing to hire anybody who was good.

Claudia: His wife was an accomplished individual.

Gary: She was accomplished. Yeah. I once read a biography of the Mitchells, and she... actually I think she had a little money even, maybe. And you know, Wesley Mitchell got his Ph.D. from the University of Chicago and as one of the first Ph.D. students at the University of Chicago, wrote a very good study on Greenbacks during the Civil War, a very important empirical study. So he was very important. Other people like Milton will know a lot more about Mitchell than I would, but he was... He set this tone of nondiscrimination, hiring the best people you could get, and that meant a lot of Jews, women, and a lot of people who were oddballs who could work at the Bureau, but they couldn't fit in to a regular type of organization.

Claudia: Did you notice in the mid-'60s that there was any dissent about Arthur? Something did happen in the '60s that led to the change, and Arthur had these various opportunities to be an ambassador, for example.

Gary: I know a little bit about that.

Claudia: Yeah.

Gary: There was a lot of unhappiness at the Bureau when Arthur accepted the chairmanship of the Council of Economic Advisers under Eisenhower. That was his first move out. I think he accepted that before I came to the Bureau, but he was there when I came to the Bureau, because I came, went down to Washington and he offered me a position on the council, but he said if he was me, he wouldn't take that. He would become an academic, go to Columbia. I had a Columbia offer. He said, "If I was you, I would take the Columbia offer. I think you'll do better in that offer." So he was away at that time.

He came back to the Bureau and he became the president, a new title they created for him, and he was clearly the boss. I mean, he has the strongest personality, strongest mind compared to people like Moore and Fabricant. He was just more able and more forceful than they were, but then he began, you know, and for a while, starting like when he came back, I don't know, '58, I don't remember, the next seven years he was very important, exercising... I said, he turned down the law and economics, he pushed the study of the Soviet economy by [G. Warren] Nutter and we had a meeting about it and I opposed it. I said, "It's too far removed from our competence, so why should we be going into that?" And Burns, well, the Rockefeller brothers, I think, were the ones financing it and that so, my opposition, I mean, it wasn't a vote, you know? Other people opposed it, but he decided to go... and I think in retrospect, he was right. I think it was an important study because Nutter didn't know Russian. I think he put together Gale Johnson, Arcadius Kahan and other

people who were active in it. I think they were much closer to the truth in a lot of the estimates that were being made about growth and so and so.

So I think that was an important study, but he exercised a very strong will, but then he began to tire of it. He began to get more into politics, you know, and they were then looking for a new director, and there was nobody natural within the New York office. He may have asked me, I think he did and I said I wasn't interested in becoming director. I think they asked George Stigler, and Stigler thought about it for a while, and I think he would have accepted it if it had been the right terms. I don't know the history of that 100 percent, but eventually, he wanted to do something like that, Stigler, but eventually he turned it down, and maybe they never even made him an official offer. So they had nobody to do it and that's when they brought John Meyer in.

Larry: so what year was...?

Claudia: '66, '67.

Gary: Well, you would know the year. [redacted] He would come in, and I don't think he knew what direction he wanted it to go in, and so on. He didn't bother our center. He supported us, but I didn't think he was good. On the other hand, I saw the problem. There was no natural person in the New York office who could head up the Bureau. But so was there unhappiness with Arthur? He dominated everybody. There was no room for... I don't know what Sol Fabricant and Geoffrey Moore thought of Arthur, but they worshiped him. People like me and Jacob and Victor, well, Arthur wasn't bothering us, so we continued to do and have increasing influence on what was being done and eventually Arthur kind of pulled himself out. He was not happy with Meyer, the choice of Meyer, when it eventually worked out. He was not happy the way Meyer worked out. I know that, because he had told me that. But he began to take a lesson...

Claudia: I think he went... I said ambassador. He went to the board at that point.

Gary: He went to the board. Yeah, he became ambassador much later.

Larry: After he was on the Federal Reserve Board he became ambassador to Germany.

Gary: But that was much, much, much later. So when he was on the board, of course, he may have asked the Bureau to prepare some numbers for him, as some other government agencies did. Leading indicators were part of the Bureau. The Bureau started leading indicators. Geoffrey Moore and Arthur Burns really started it, and Jeffrey Moore took it over and eventually the government took over what the Bureau had pioneered.

Larry: And eventually the government privatized it again.

Gary: And eventually they privatized it again, exactly. So he may have asked the Bureau to compute various numbers for him when he was chairman but that I don't know. I wasn't involved in that. So was there unhappiness with Arthur? Probably, but that, I don't think, played a role in any of these types of decisions. He dominated the

board. He dominated everybody around there, and he was... Arthur Burns, you probably never knew him, was a very forceful...

Claudia: I did know him.

Gary: Oh, you did?

Claudia: Late in his life, and I was charmed by Arthur, and I was told that all women are charmed by Arthur, and men are dominated by him.

Gary: Well, he had a very strong personality. He had a very strong personality. He once told me — again, I don't know if this should be for public statement — that when he went to Washington, he said he felt, he said there were a lot of economists in Washington and knew what he had to do. He said he felt like he could tie one hand behind his back and handle all the economists that were there, and he did. He was a very important... When he was with the council and so on, he had a very — and at the Federal Reserve — he dominated the Washington economic government, economic scene, but he was a very strong person. Very smart, I had a lot of respect for Arthur Burns. And the only things I was down on him about — he was too anti-theory. I think that was a mistake and he... that's why he created that tradition at Columbia for people he taught, and he had a too conventional view of what economics should be like. But to his credit, he eventually began to support our work, but otherwise, I was a big fan of Arthur Burns. Did we answer all the questions?

Claudia: There was something that you were going to come back to, but I forget what it is. Oh, Kuznets?

Larry: Right.

Gary: Oh, that's a small thing, but it might be interesting. The Bureau had the policy — It wasn't just a tradition it was a policy — that if anybody submitted a manuscript, it would have to be read by two to three Bureau senior staff members. So Simon Kuznets submitted a manuscript on long-term investment, I mean, you would know what he did better, but it was something like that, and I was asked to read it as one of the three readers. I spent a lot of time reading it, and wrote like an eight-page report, very critical of some of it. And I said, "Well, he puts together some interesting data, but then he has the last chapter of speculation." I said, "There's absolutely no connection between the speculation and the data he compiled," and I tried to demonstrate that. I said, "While the speculation may be interesting, he gives the impressions it's following his data collection, and there's no connection here," and I went through case by case, cited it and I said, "Look, this is what he finds, this is what he's speculating about. How can you go from this to this to that?" I said, "So I think," I mean, I was then... This was early. I was probably... I was still in my 20s, I mean, and I said, "I think this thing has to be rewritten to do..." so and so. So I sent it in to Arthur, and Arthur sent it on to Kuznets, and I heard from Arthur that Kuznets said, "Who's this young upstart who's telling" him what he should be doing and so on, and I don't know how much changes he ever made. I mean, Kuznets was already an important economist. I don't know how much he made.

So I think Kuznets, when he first met me, kind of had an, after that, had a negative

view, but eventually he and I began to be friendly, and we would always speak about things, and when he retired from Harvard, he was telling me how, "Don't retire," he said. He had to retire because that was the old rules — "Don't retire," he said. "Retirement's a real problem. You don't know what to do with half your time now. You were on committees and so on before. You can only do research a certain number of hours a day." I thought it was very good advice. Maybe I've followed that advice in my own career. As long as you're active and so on as he was, I think retirement was not a good option for him. He should have continued as full time.

So I had good relations with Kuznets eventually, but I know he was very unhappy over this report. On the other hand, I think, in retrospect, my report was right. I mean, I think the points I made were right. And I think there was a tendency sometimes in what Simon wrote to have a weak connection between some of, you might say, the theory or the speculations and the data side, and I think it was worth pointing that out in this Bureau vine. So I mean, I wouldn't retract what I said. Maybe being young, I would've said it in a more tactful fashion now, but I think I was right on that point. Eventually, we had good relations. But no, I wouldn't say I was ever really close with him, because he was at Penn and then he went to Hopkins and then he went to Harvard, and his coming into the Bureau became less and less frequent.

Claudia: Right. And in some sense, some ironic sense, Simon was probably more theoretical than anyone at the Bureau until the mid-'50s when you and Jacob came in.

Gary: Absolutely. He had a strong theoretical bent to him. He did a lot of interesting projects. He was interested in human capital. You read some of his stuff on growth and interesting human capital work.

Claudia: Sure. Well, one of the big blowups at the Bureau, way before you arrived, was when he proposed to Burns to have a comparative economic growth project in '47, I think, when Burns just took over for Mitchell, and Burns said, "Absolutely not," and Simon essentially leaves the Bureau in many ways.

Gary: Well, not only for that reason.

Claudia: Yeah.

Gary: You know, they were competitive...

Claudia: They were competitive.

Gary: For the head, and Burns went out, and I think that was one of the reasons...

Larry: That's the unspoken thing no one ever says.

Claudia: Right.

Gary: Right, but I think, there's no question that was true, that they were competitive for that position.

Claudia: There was an animus between the two of them that no one could put their finger on, but a true animus that is very...

Gary: There's no question. I had one small experience about that.

Claudia: We're going to run out of tape in the middle of this, but that's okay.

Gary: All right, well this is not that important, but you and Larry might be interested.

Claudia: Yeah.

Gary: I was preparing a session for the American Economic Association on some topic, and Burns had asked me to prepare, and Burns was then president of the association, and I suggested to Burns that I was going to invite Simon Kuznets. He said, "Well, yes, he'd be good for that, but if you invite him, he won't accept. Better if I give him a call and invite him, because you're just a young guy and status is important to Kuznets." So he did call him. He did invite him. He did accept. So I read between the lines at that time, from what Burns said, that there was something. And I heard about that they were competitive from Mitchell. So they were competitors. A lot of people are competitive in every profession, when they were a comparable age, they were both foreign-born, came over as immigrants to this country. So they had similar backgrounds in a lot of ways, although their paths diverged.

Claudia: Let me turn this thing off now.